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NOTE—This Journal offers prizes for the best studies of tornadoes. Competition closes July 1st, 1889. For particulars send to Mr. Harrington, at Ann Arbor, for special circular.

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THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. V. ANN ARBOR, APRIL, 1889. No. 12.

ORIGINAL ARTICLES.

THE RED SUNSET SKIES OF 1884-1885.

PREFATORY NOTE.

The detailed observations on the red-sunsets and sunrises and Bishop's ring from April, 1885, to February, 1887, were partly reported upon in a short paper that I offered in November, 1885, to the judges in the matter of the Warner Prize Essay. As other work has prevented me hitherto from further deductions from my observations, and as the exhaustive work of Kiessling and of the committee of the Royal Society will now bring to a temporary close the discussion of this interesting and important subject, I have concluded to offer my essay for publication, with no material change from the form in which it left my hands in November, 1885 — words added since then being in brackets. We thus complete the literature of the subject, and the better perceive what change has taken place in our views during the past four years. It is proper to add that the appearance of pink glows at the characteristic times and positions still continues, and occasionally, under favorable circumstances, these are as brilliant as the fainter ones of 1885. I have therefore concluded (1) that vapor haze is more important than dust haze; (2) that a shallow layer, sparsely filled with such minute particles of vapor haze generally accompanies every area of high pressure and clear air and appears to produce the diffraction necessary for the present phenomena; (3) that a deeper layer, more densely filled with minute, and also with still larger, particles suffices to explain the excessive phenomena of 1883-'4; (4) that the dust and haze needed to produce red coloration of light by selective absorption and reflection is always present in the lowest air stratum; (5) that the Krakatoa eruption sufficed to throw sufficient moisture into the atmosphere to explain the diffrac-

tion phenomena of 1883-'84 and its gradual subsidence since then, so that the accumulation of vapor of meteoric dust is an unnecessary hypothesis; (6) that the daily weather report for the northern hemisphere printed in the S. S. Bull. Int. Sim. Obs. shows that the distribution of the Krakatoa vapor must have been largely influenced by disturbances in the lower amosphere, and that we do not need to assume an exclusive or even predominating influence of general upper currents, either easterly or westerly.

CLEVELAND ABBE.

February 19, 1889.

In the autumn of 1883 there gradually spread over the northern hemisphere a daily succession of remarkable, brilliantly-colored twilight phenomena, such as were not remembered to have been before seen by any living person. For an unusually long time after sun-set the western sky glowed with red and purple and pink tints, as if the horizon were a mass of flame. Attention was universally riveted upon this glorious display of color; and as the scientific observers were unable at first to suggest any plausible explanation, the credulous and superstitious denizens of the globe allowed their fears and imagination full play. Telegraphic reports from all parts of the world showed the universality of the phenomenon over the northern hemisphere. The first inclination to attribute this phenomenon to extraordinary atmospheric or meteorological changes, and the subsequent suggestions that tails of comets or solar eruptions or other cosmic matter was concerned in this phenomenon, were soon replaced by a general adoption of the idea that in some way they were dependent upon the grand eruption of the volcanic island of Krakatoa, in the straits of Sunda. By some, the volcanic ashes or dust, by others the aqueous vapor, and by others the novel gases erupted from this volcano, were supposed to be the precise occasion of the red sunset phenomena, and many were the suggestions as to how these erupted matters were supported in and disseminated throughout the atmosphere and as to the precise mode of their action upon the sun's light.

It was, of course, confidently expected that as these foreign particles were deposited from the upper regions of the air, the special sunset phenomena would gradually disappear; and to a

certain extent they did diminish in intensity, so that by January or February, 1884, they may be considered to have disappeared.

During the summer of 1884, attention began to be called, and especially in Switzerland, to the remarkable silver-white ring with dusky and ruddy edges surrounding the sun day after day with an outside diameter of 15 to 20 degrees. Perhaps the novelty of this phenomenon would have escaped attention on account of the rarity of records relating to this class of phenomena, since but few make a regular practice of observing the halos around the sun according to the methods that have been recommended since the days of Sir Isaac Newton. However, as the existence of this ring during 1884 is beyond all dispute, we accept the assurance of the Swiss mountaineers, to the effect that it is a novel phenomenon which made its first appearance early in the year 1884. The earliest record of this ring, September, 1883, is usually attributed to Mr. S. E. Bishop, of Honolulu, and it is properly called by his name, as has been suggested by F. A. Forel. While attention began thus to be directed toward Bishop's ring as being possibly a residual phenomenon of the red sunsets, suddenly the latter reappeared, and it was found that both ring and sunsets could exist simultaneously. This second annual reappearance was heralded in England about September 10th, 1884, or ten days earlier than in 1883, and in general, the brightness which came to its maximum between the 20th of December and the 10th of January, 1884, was both later than, and inferior to, that of the first appearance. This second annual display closed in January, 1885, and may be considered to have powerfully contributed to dispel the idea that the sunset glows originated in the Krakatoa eruption.

The Bishop's ring during 1885 continued in general undiminished, but with occasional apparent temporary fluctuations in brightness up to the present time. On the other hand the red sunset phenomena, after becoming invisible throughout the summer of 1885, have again become prominent, beginning early in August, in the United States.

It is evident, therefore, that any description of the sunset phenomena for the fourteen months preceding January, 1885, as

required by the terms of competition, would be quite imperfect [as a record of the whole] since at the present time (November, 1885) we are in possession of more recent records of its reappearance. I have therefore in the present essay considered all the results of observations up to the present time in order to arrive at a reasonable explanation of the cause of the long series of red sunset skies.

In August, 1885, without realizing at that time the announcement made by Mr. H. H. Warner in January, 1885, and animated only by a desire to be more familiar with the phenomena, and if possible to ascertain something of the cause, I began a series of careful records which, although wanting in the accuracy that would have been secured by the use of proper instruments of measurement, have at least, as I hope, contributed to fix the dates, times, and general features of the phenomena as they have been observed from day to day by me in this city (Washington). On very few occasions have I failed to examine the sky before sunrise and after sunset from August 14th to November 28th, inclusive.

The journal of observations made by myself, with the corresponding computations, justifies the following conclusions as to the twilight phenomena of the autumn of 1885:

(A) Throughout the summer and autumn (of 1885) the Bishop's circle has always surrounded the sun; as seen on the clearest days between 9 A. M. and 3 P. M., this has consisted of a pearly white circle surrounded by a dusky, colored ring; the white circle has about eight degrees radius when the sky is clearest and bluest, and the radius is measured from the sun upwards, but more than this, possibly even 12 degrees radius, when measured in duller skies or at low altitudes of the sun; especially does this circle and its outer ring enlarge and vary with the smoky condition of the lower atmosphere over a city. The dusky ring separating the inner white circle from the outer blue sky generally shows a radial striation, it merges into the inner white more abruptly than into the outer blue, its width is about six degrees in the clearest blue sky, or its radii are eight degrees for the inner and fourteen degrees for the outer limit,

which radii may become twelve and eighteen degrees respectively in other conditions of the sky; on clear days this ring generally has a decided red or brownish-red tint.

(B) The twilight phenomena which ordinarily consists of a regular succession of sky colors (not cloud colors) beginning at sunset with a red band near the horizon above the sun, and which gradually changes to orange, yellow, and finally white, has now at the close of twilight a second stage super-added, which began about the 10th of August, 1885, and which consists essentially of the following three items: (1) A special red, pink, rose or purple glow appearing about 16 minutes after sunset and ending about 32 minutes after sunset, with a maximum brilliancy four or five minutes before its end; (2) a second special red glow sometimes finer but generally fainter than the first, appearing about 48 minutes after sunset and ending at 64 minutes; (3) a third glow which at its beginning often merges with the second, and which continues until the last trace of color disappears in the west, thereby converting the white light of the last part of a normal sunset into a pale reddish light. These three glows vary, as to the time of appearance, of maximum, and of disappearance, always in the same ratio, so that the intervals after the sunset maintain very nearly the ratio of 1, 2, 3; and when the absolute intervals diminish the actual durations for each glow slightly increase. The intervals are larger in August than in November, but this is owing to the declination of the sun, since if we compute the angular depression of the sun below the west horizon the values remain nearly the same, namely, about 3°, 6°, 9°, and 12° of arc, corresponding to the 16, 32, 48 and 64 minutes of time. The apparent elevation of these red areas above the sunset point in the west horizon, are:

For first glow: lower edge, 15°; upper edge, 40°; maximum intensity at 20°.

For second glow: lower edge, 10°; upper edge, 20°; maximum intensity at 12°.

For third glow: lower edge, 2°; upper edge, 4°; maximum intensity at 3°.

(C) The red glows thus added to the normal twilight occur in

the morning precisely as in the evening; the intensities are different, but the ratios remain 1, 2, 3.

(D) The careful study of my detailed observations of the shadows and streamers that surmount the hidden sun demonstrates that the red glow is invisible when the direct sunlight is cut off from a certain stratum in the lower portion of the atmosphere. Thus, at an apparent altitude of from 8° to 20° the observer sees the pink glow X merging into white, then below it yellow at Z Z and then red at the horizon Y, but interrupted by a shadow U extending from within less than 1° of the horizon up to the limit of the pink, and within which he sees blue at S between the pink at X X and instead of the yellow at Z Z he sees green at T while the red at the horizon beneath Y Y is almost identical with the red in the shadow at U.

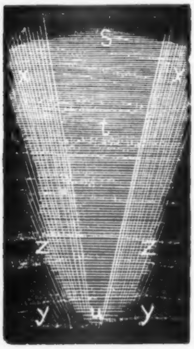


FIG. 1.

(E) The comparison of twilight observations with the tri-daily weather map shows that the above shadows may be reasonably concluded to be the shadows of clouds near the observer, namely, within 50 or 100 miles, and not the shadows of mountains at a still greater distance; for example, on October 22, seventeen minutes after sunset in a sky cloudless except a few cirri, a vertical shadow appeared over the sun, which in four minutes disappeared, and was replaced by the full rose tint. If this shadow were that of a mountain, such as the peaks of hills in the Appallachian range the mountain would need to be over four degrees, namely, over 200 miles distant from him, west of the observer, near his parallel of latitude, but if it proceed from a small cloud much higher than the low mountains of this region and more nearly on a level with the origin of the rosy light, then it may be located much nearer to the observer.

(F) The comparison with the weather map also shows that we have no fine display of pink and certainly no second or third glow when the region beyond the western horizon is cloudy

although we may have such when the region within 20 miles to the westward is covered with detached masses of cloud.

(G) The tints described by me as pink, rose, purple, and red glows, are very different from the orange and salmon tints reflected from the under surface of a cloud when the sun's rays strike up from below; the latter is a rich compound color, the remnant of the spectrum after some parts have been absorbed in the transmision through the lowest portion of the atmosphere, of a beam of direct sunlight, and which filtered beam striking the cloud particles is reflected to the observer's eye with but slight alteration. The former is an almost pure specrum tint, diluted with either white or yellow or blue according to circumstances, but never more complex than a secondary color.

(H) My observations on the polarization of this pink glow although taken with crude apparatus yet suffice to show that if any polarized red light exists it is not nearly so large a percentage as for the blue sky at the same distance from the sun or in other words that the polarization of the red light corresponds in amount to what belongs to light coming from particles much nearer the sun [in apparent angular distance] than is the apparent location of the glow.

(I) The Bishop's ring shows, to me, no trace of polarization either in its white center or its dusky limiting ring, but Cornu, in 1884, with much better apparatus, finds a slight percentage of polarized light, only not so much as the blue sky would have shown.

In explanation of the preceding phenomena, a number of hypotheses have been suggested. For our present purpose we need only say that every one is agreed on the conclusion that some foreign substance, either gas, or vapor, or dust, has come in between the observer and the sun. This substance must modify the light of the sun and produce the abnormal twilight phenomena by some one of the optical processes known as absorption, reflection, selective-absorption, selective-reflection, refraction and dispersion, diffraction, or polarization. With regard to the origin of this foreign matter, the various hypotheses may be characterized as: (1) The Kraka-

toa Eruption of August 26th, 1883; (2) General Volcanic Dust; (3) The Smoke of Prairie Fires; (4) Meteoric, Cometary, or Cosmic Dust; (5) Some special conditions of Atmospheric Moisture.

Others have looked for the origin of the red light in the hydrogen flames of the Solar Atmosphere, and still others have been content to assert that the special phenomenon of 1883 was only a well developed normal twilight.

To me it seems more proper to recognize the fact that the presence of a layer of dust or haze in the upper atmosphere will at any time cause phenomena, such as those observed within the past few years, and as more or less of such dust is always present, these red glows may properly be considered as a part of the normal twilight phenomenon, but it becomes still necessary for us to offer some reasonable hypothesis as to why they should have been so remarkably well developed within the last few years.

With regard to the origin of the dust or vapor, whichever it be that causes the red glow, the Krakatoa hypothesis certainly had, for a time, much to recommend it. It was acknowledged that this eruption, which took place on the 26th of August, 1883, was by all means the most violent on record. Clouds of steam and ashes were belched to a height, computed at first to be about 17 miles above the earth's surface, although this computation seems to the present writer to be based on data and assumptions so uncertain as to justify asserting that this cloud need not have attained an altitude greater than six or seven miles. It was at first suggested that the matter projected 15 or 20 miles above the earth would meet the great [hypothetical] easterly current prevailing at that height, where the still air does not partake of the rapid easterly motion of the whirling earth, and consequently to observers on the earth, would appear to have a rapid [relative] movement from the east, at a rate of possibly several hundred miles per hour. It was thus that the successive appearance [to the westward] of the [red] sunsets within a few days after the 26th of August, in India, Africa, Trinidad, Panama, Honolulu, were supposed to be accounted

for. But we have no evidence whatever as to the existence of such a rapid upper current. No mathematical or mechanical laws require its existence, and what little we know of upper currents in the atmosphere from the paths described by the dust and sparks from falling meteors, shows that there is as great a variety of winds in the higher strata as in any other part of the atmosphere. Again, this hypothesis, if accepted, in order to explain the phenomena near the equator, does not without further assumptions explain its subsequent spread northwards, away from the equator to the north temperate zone. In fact, sufficient observations are at hand to show that the present series of sunsets began some time before the eruption of Krakatoa, to meet which consideration it is necessary to assume that other volcanoes in other parts of the world must have contributed their smoke and steam to the phenomena. In general it must be allowed, that earlier accounts of similar phenomena, such as the red sunset in Selbourne, June 23rd, to July 20th, 1783, in England, 1821, and Malta in 1831, were all preceded by volcanic eruptions in more or less distant parts of the earth; but as eruptions are frequent, it is not difficult to find one that may be said to have preceded almost any remarkable phenomena; so that these coincidences, instead of being accepted as a proof of the interdependence of the two phenomena, must be quietly passed by as cases of simple chronological coincidence.

The least objectionable hypothesis, perfectly accordant with previous experience, and, as will be seen, better explaining the special phenomenon of the past three years, is the assumption that in August, 1883, the earth began plunging into successive clouds of meteoric dust, thereby adding to its upper atmosphere a slight mass of solid particles distributed through an immense volume of air. The mere presence, in 1883, of this new foreign matter is not enough to explain the reappearance of red sunsets in 1884-85. These reappearances show that there must exist in the neighborhood of that part of the earth's orbit through which it passes during the period from August to March a sufficient number of meteoric clouds, or more likely a stream of meteoric

matter such that the earth shall annually renew this layer of dust. If the orbit of this meteor stream contains several regions of alternate scarcity and frequency of cosmic matter, then we shall have intervals of years without red sunsets, and of others with that phenomenon, alternating in some systematic manner. According to this modification of the dust theory, the dust once dissipated through our atmosphere, floats there both by reason of its minute dimensions and also by means of the buoyant effect of minute hot air currents streaming upwards from each atom when the sun's rays strike it; it is, however, not necessary to assume that the dust layer is entirely floating within our atmosphere; observations must decide as to whether it may not be located so far above the earth as to maintain its uninterrupted orbit around the sun. In either case we have as a result an extensive cloud of fine particles between the observer and the sun. (The immense volume of dust required to explain the existence of the red sunsets over the whole earth can only be obtained by this appeal to cosmic matter. If we should for a moment consider the vapor and dust cloud that floated above Krakatoa on the 26th and 29th of August, 1883, and as an extreme limit allow that it contained 5,000 cubic miles, or could have covered one square degree of the earth's surface one mile deep, and then this, spread over the 41,256 square degrees of the globe, gives us a layer only 1.5 inches deep; but nine-tenths of this cloud was deposited as heavy dust and rain within a few days after the eruption, and it would be an extreme assumption to assume that even the remaining tenth part of these 5,000 cubic miles extended up into the higher regions into the influence of the hypothetical and very problematical swift eastern current).

A consequence of the inter-position between the observer and the sun of a cloud of pretty uniformly sized dust particles is the formation by diffraction of a series of concentric colored rings about the sun, phenomena that have been first studied by Young, Fresnel, Fraunhofer, etc. The last has shown in his theory of coronæ, halos, etc., that for red light the radii R_1, R_2, R_3, of the consecutive red rings is as given by the following formulæ,

where d is the diameter of the dust particles expressed in Paris inches, and where 0.27 degrees represents the radius of the disk of the sun:

$$R_1 = 0.27^\circ + \frac{0.000\ 0257}{d} \times 57.3^\circ$$

$$R_2 = 0.27^\circ + \left\{ \frac{0.000\ 0257}{d} + \frac{0.000\ 0214}{d} \right\} \times 57.3$$

$$R_3 = 0.27^\circ + \left\{ \frac{0.000\ 0257}{d} + 2\,\frac{0.000\ 0214}{d} \right\} \times 57.3$$

From this formula I have computed the following table giving the radii of the three first diffraction rings for a series of assumed values of the diameter of the particles:

d Paris inches.	R_1 °	R_2 °	R_3 °
0.001285	1.42	2.38	3.34
.000642	2.57	4.49	6.41
428	3.72	6.59	9.46
321	4.87	8.70	11 53
257	6.02	10 81	15 60
214	7.17	12.92	18.67
184	8.32	15.03	21.74
161	9.47	17.14	24.81
143	10.62	19.25	27 88
128	11.77	21.35	30.93
117	12.92	23.46	34.00
107	14.07	25.57	37.17
099	15.22	27.68	40.14
092	16.37	29.79	43.21
086	17.52	31.89	46.26
080	18.77	34.19	49.61

By the above table and formula Kaemtz and others have computed the average size of the particles that form ordinary diffraction coronæ, but before doing this in the present case we have to determine whether the dusky Bishop's ring [may be assumed to be a diffraction ring and whether it] is of the first, second, third, or higher order.

The diffraction rings that should be caused by the hypothetic dust may plausibly be expected to be too faint to be easily seen in full daylight; even those far brighter ones, due to the ordinary vapor of the atmosphere, are invisible in the glare of the sunlight. They, like the Bishop's ring, become much more striking when the sun is well obscured by a thick cloud with

sharply defined edges, and the conditions may also be favorable after sunset when the sun is hidden by the solid earth. At this time the rays BB′ from the sun at SS passing through the earth's

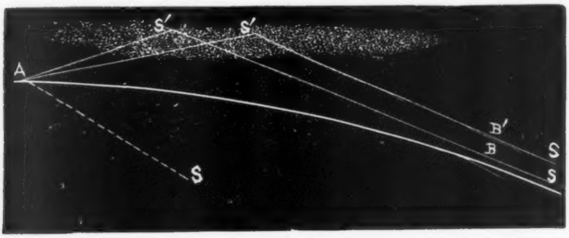


FIG. 2.

atmosphere, especially its lower portions, strike the dust cloud at S′S′; were the earth and atmosphere away, and the spherical dust envelope only left, the observer at A, looking toward the sun at S, should see a series of concentric rings, colored red inside and blue outside, as in ordinary diffraction phenomena. But if the atmosphere be present, the solar rays, after transmission, are decidedly red, and thus the red light that falls on S′S′ is far more homogeneous than the original sunlight. The observer at A will, therefore, see at S′ the diffraction rings due to the red light, and as the initial red light is not perfectly homogeneous, it will by diffraction be so analyzed that the portion of the red circle nearer the sun will be yellowest, and the portion furthest from the sun wíll be bluer or purpler than the central region, which precisely agrees with my observations. At and after sunset this red band would appear to surround the sun, were it not that other twilight phenomena are at hand that obliterate it from sight before it can be traced very far; nevertheless, observation invariably gives it the outline shown in the figure; namely, a decided circular limit on the side nearest the sun, the circular band S′ S′ S′ reaching down on

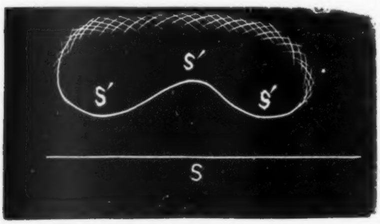


FIG. 3.

either side to near the horizon until obliterated by the bright yellow or white twilight colors. The angular distance from S, the sun below the horizon, to S′ above, is the radius of the diffraction ring due to the dust cloud.

As the sun sinks lower, the angle between S′ and S increases until it corresponds to the dark space between two consecutive rings, when the red glow at S′ from the dust region is replaced by the normal twilight sky. If the dust region is relatively low down this normal twilight sky will be the yellow and white of light reflected from vapor and dust particles, and this I have usually observed to be the case in the first glow. My observations of cloud shadows show that if at any time in the first, second, or third glow the light be cut off from S′S′, then the blue sky and the green and yellow reappear precisely as in other parts of the sky. I therefore conclude that the red glows (occurring first at 16 to 32 minutes; second, 48 to 64 minutes; and third, 64 minutes and more after sunset, or having their maxima when the sun is respectively 4, 8, and 12 minutes below the horizon, and having their bands of maximum brilliancy elevated above the west horizon 20 degrees, 12 degrees, and 3 degrees, respectively, as given above in section B,) are diffraction rings whose radii from the sun's center to the maximum red are 24 degrees, 20 degrees, and 15 degrees, respectively, for the first, second, and third glow.

If this conclusion is correct it follows that the first glow is due to the visibility of the third or outer diffraction ring which becomes visible first after sunset, while the first or inner and brightest ring becomes visible only at the close of twilight. That the observer has not seen this first and brightest ring close to the sun immediately after sunset is explicable, and sufficiently so, by the overpowering light in that region sent to him by the lowest portion of our atmosphere. That he does eventually see this first ring at the close of twilight is due to the removal of all the illumination of the lower air between himself and the west horizon. But that he does not then also see all the three bands simultaneously is due to the fact that the second and third are intrinsically fainter than the first ring, and

all are now at a much greater distance from him than when the first glow appeared at 16 minutes after sunset; therefore, all are seen through so great a layer of the thick lower atmosphere, that only the first remains faintly visible. (It must be allowed that observations in cities are so affected by the illumination of city smoke and dust, by city gas and electric light, that my record of altitudes and times of observing the last visible trace of the third glow are liable to be quite crude.)

The following diagram shows the positions of the sun and the red glows with the hypothetical dust cloud at the moments when the successive diffraction rings become visible to the observer.

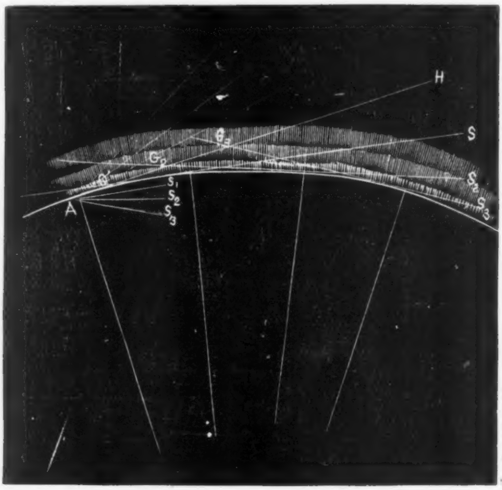


FIG. 4.

It is above stated that the radii of these rings must be about 24, 20, and 15 degrees. With these data we may now obtain from the table above given the size of the particles that must give rise to such diffraction phenomena, we thus obtain:

For the first glow or third red diffraction ring of 24° radius the diameter of the particles	0.000192	Paris inch
For the second glow or second ring, 20° radius ...	0.000166	" "
For the third glow and first ring radius, 15° ...	0.000101	" "
Average, ...	0.000153	" "

For this average size of particles the respective radii would have been 10 degrees, 18 degrees, and 26 degrees, thus showing that the greatest discrepancy is, as might have been expected, in the determination of the first ring.

We now return to the dusky red Bishop's ring seen in the daytime around the sun, and conclude at once that its observed diameter, averaging 15 degrees and always included between 12 and 18, exactly corresponds to the third sunset glow or the first and brightest diffraction ring; as the measurements made with the sextant of the limits of this dusky ring must be more accurate than the results of difficult twilight observations, it would be preferable to adopt 15 degrees as its mean radius and 0.000101 Paris inches as the mean diameter of its particles. This, however likely it may be for the daytime when we see through the highest and thinnest part of the layer of haze, must give us too large a ring and too small sized particles for use in explaining the twilight observations when we look through the lower and denser mass of the dust region. In other words, if the twilight observations give 10 degrees radius on the average as appropriate to the larger particles, the midday observations give 15 degrees radius as appropriate to the smaller particles; this consideration would require that the mean diameters of the second and third rings at midday should be as great as 28 and 40 degrees respectively, which will also imply that they be very faint. But these dimensions apparently vary from day to day with every variation in meteoric clouds. If, moreover, there is considerable mixture of particles of various sizes without giving any one size predominance, there will then be an interference of successive red and dark rings, thus apparently explaining the general dusky appearance of the red ring.

It will be observed that the dimensions of the dust or vapor particles are much smaller than those computed by Kaemtz, for the ordinary cirrus cloud haze which gives rise to the small solar and lunar coronæ; this of course is entirely in accordance with either hypothesis as to the nature of the particles, namely, as to whether they are transparent vapor spheres or non-transparent globules of meteoric matter. None of the phenomena

that have thus far been observed imply any such dispersion of light as would be inevitable if the rays passed through small drops of liquid, but are explicable as diffraction phenomena in which the nature of the subtance is immaterial.

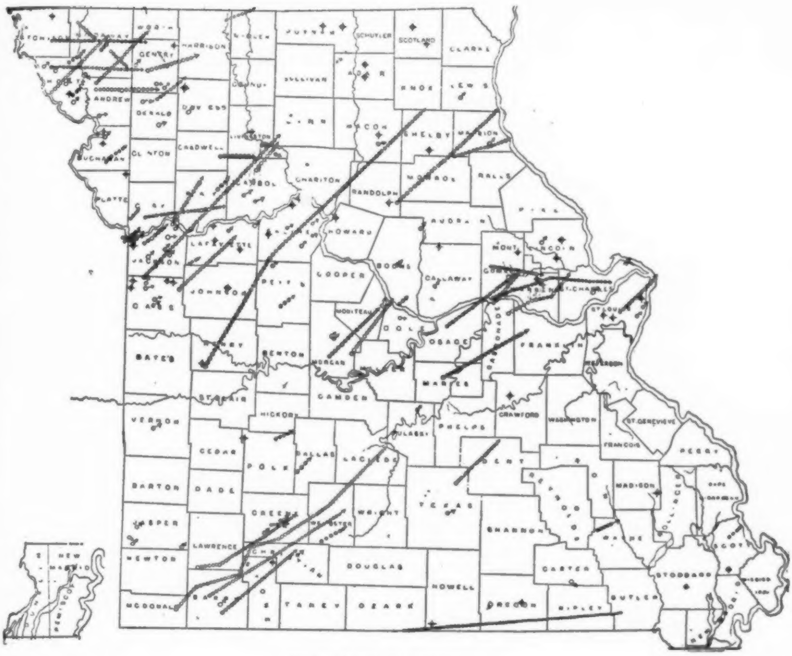
We infer from these considerations that the Bishop ring is due to particles so far removed from the earth's surface as to suffer very little change from day to day, remaining sensibly the same throughout all seasons, while the red twilights are due to similar diffraction rings by similar and slightly larger particles added to the earth's atmosphere annually as it enters the meteoric ring, and the larger individuals of which soon settle down into the earth's lower atmosphere, while the smaller particles remain higher up and contribute to the permanence of the Bishop's ring.

The distribution of the proper clouds of dust throughout the earth's atmosphere is apparently quite irregular as to altitude and geographical extent, depending on the atmospheric currents as well as on the occurrence and location of showers of meteoric dust.

WHAT IS A TORNADO?—The Signal Service defines a tornado officially as follows:

"In order to define more exactly the complex meteorological phenomena known as Tornadoes, it is directed that hereafter no observer of the Signal Corps shall note in his official reports to this office any storm as a tornado, unless it shall be a violent local storm in connection with which is noted (by day) a well-defined pendent funnel-shaped cloud, with attendant rotary winds of sufficient violence, over a well-marked path, to uproot trees, prostrate dwellings, carry heavy objects long distances, or otherwise leave plain evidence of unusually violent and rotary wind currents."

The essential feature of this definition is the characteristic cloud funnel. The requirement as to damage done evidently is simply an earmark to indicate to the observer whether that particular storm is worth reporting or not.



TORNADOES IN MISSOURI.

STATE TORNADO CHARTS.—MISSOURI.

BY LIEUT. JNO. P. FINLEY, SIGNAL SERVICE, U. S. A.

TABLE I.—*Tornadoes in Missouri.*

Period of observation, 75 years, 1814-1888.
Total number of storms, 169.
Year of greatest frequency, 1883,—34 storms.
Month of greatest frequency, May,—42 storms.
Day of greatest frequency, April 18th,—16 storms.
Hour of greatest frequency, 5 to 6 P. M.
Month without storms, January.
Prevailing direction of storm movement, NE.
Region of maximum storm frequency, central and northwest portions.

TABLE II.—*A Chronological Table, showing the location, date, and time of occurrence and general character of formation and movement of Tornadoes in the State of Missouri for a period of 75 years, from 1814 to 1888.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Jackson.	"Spring"	1814	....	NE.	....	....
Jackson	"Summer."	1826	....	NE.	Funnel.	....
Jackson	October 25.	1844	9 p. m.	NE.	"	1,500 to 2,100.
LaFayette?	"Spring"	1849	Evening.	....	"	....
DeKalb	June 3.	1860	Evening	ENE.	"	....
Polk	June 16.	1863	Afternoon.	NE.	"	....
Morgan	May 26.	1870	....	NE.	"	150.
Scotland	August 8.	1870	....	NE.	"	....
St. Louis	March 29.	1872	8:30 p. m	NE.	"	....
Morgan	May 28.	1872	....	NE.	"	2,640 to 3,960.
Cole	....	1873	2:30 p. m.	SE.	"	150 to 200.
Johnson	February 23.	1875	Afternoon.	NE.	"	....
Pettis	February 23.	1875	3:40 p. m.	NE.	"	500 to 900.
Buchanan	July 3.	1875	Afternoon.	NE.	"	....
St. Charles	February 27.	1876	3 p. m.	NE.	"	....
Monroe	March 10.	1876	4 p. m.	NE.	"	600.
Adair	April 12.	1876	Afternoon.	NE.	"	....
Cass	July 20.	1876	4 p. m.	ENE.	"	800 to 1,500.
St. Louis	July 28.	1876	Morning.	NE.	"	....
Clinton and Platte	Au,ust 19.	1876	1 p. m.	NE.	"	....
St. Louis	August 29.	1876	3:30 p. m.	....	....	....
Barry	March 31.	1877	5 p. m.	NE.	Funnel.	300 to 500.
LaFayette	March 31.	1877	....	NE.	"	....
Buchanan	June 24.	1877	....	NE.	"	....
St Louis	June 30.	1877	Afternoon.	NE.	"	....
Scott	April 23.	1878	3:30 a. m.	....	"	....
Gentry	May 17.	1878	11:45 a. m.	NE.	....	....
Stoddard	May 18.	1878	7:35 p. m.	NE.	Funnel.	....
St Louis	May 18.	1878	12:50 p. m.	....	"	....
Cass	May 24.	1878	4:45 p. m.	NE.	"	....
Clay and Ray	June 1.	1878	3 p. m.	NE.	"	600 to 1,000.
Buchanan	June 14.	1878	....	NE.	....	....
Cass	July 16.	1878	2 p. m.	NE.	Funnel.	80 to 100.
St Louis	April 14.	1879	....	NE.	....	....
Nodaway	May 29.	1879	3:30 p. m.	NE.	Funnel.	300 to 3,600.
Holt and Nodaway	May 29.	1879	6 p. m.	NE.	"	....
Andrew	May 29.	1879	3 p. m.	ENE.	"	....

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Jackson	May 30.	1879	5:30 p. m.	NE.	Funnel.	150 to 7,500.
Gentry	May 30.	1879	9 p. m.	ENE.	Funnel.	100 to 1,500.
Holt	May 30.	1879	....	NE.	....	....
Adair	May 30.	1879	....	NE.	....	....
Putnam	May 30.	1879	9:30 p m.	NE.	Funnel.	....
Jackson and Ray	May 30.	1879	Afternoon.	NNE	"	....
Cass	May 30.	1879	6:30 p. m.	N 20° E.	"	....
Jackson	May 30.	1879	Afternoon.	NE.		....
Holt	June 13	1879	....	NE.	....	....
Cass and LaFayette	October 8	1879	1 p m.	NE.	....	....
LaFayette	November 8.	1879	2:15 p. m.	NE.	....	....
Randolph	December 9.	1879	4 p. m.	NE.	Dense roll'g mass.	....
Texas and Dent	April 18.	1880	5 p m.	NE.	Funnel.	1,320 to 3,960.
Stone	April 18.	1880	Afternoon.	NE.	....	1,320.
Webster	April 18.	1880	2 p m	NE.	Funnel.	2,640 to 5,280.
Pulaski	April 18.	1880	8 p m.	NE.	Inverted cone.	150 to 900.
Morgan	April 18.	1880	5:30 p m.	NE.	Funnel.	5,280.
Crawford	April 18.	1880	Afternoon.	NE.	"	....
Cole	April 18.	1880	6 p m.	NNE.	"	150 to 600.
Cole and Morgan	April 18.	1880	5:30 p. m.	NNE.	"	7,920.
Callaway	April 18.	1880	Afternoon.	NE.	"	....
Barry, Stone, Christian, Greene and Webster	April 18.	1880	5:40 p. m.	NE.	"	600 to 1,320.
Barry, Stone, Christian and Webster	April 18.	1880	5 p. m.	NE.	"	2,640.
Lewis	April 24.	1880	Afternoon.	NE.	....	....
Jasper	April 24.	1880	5 p. m	NE.	Funnel.	....
Lincoln	May 9.	1880	1 p. m.	NE.	"	600 to 800.
Jackson	June 5.	1880	4:30 p. m.	NE.	"	....
Carroll and Livingston	June 5.	1880	Afternoon.	NE.	"	....
Cole	July 2.	1880	Between 8 and 10 a.m.	E.	"	....
Oregon	November 25.	1880	Evening.	NE.	....	....
Ripley	November 25.	1880	Evening.	NE.	Funnel.	....
Howell	November 25.	1880	Evening.	NE.	"	....
Jasper	December 4.	1880	7:20 p. m.	NE.	....	....
Lawrence and Greene	December 4.	1880	7:35 p. m.	NE.	Funnel.	500 to 900
Saline	December 4.	1880	9:30 p m.	NE.	"	....
Stone, Ozark, Christian and Barry	December 4.	1880	8 p. m.	NE.	"	....
Saline	April 18.	1881	....	NE.	"	....
Lincoln	May 5.	1881	....	NE.	"	....

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Nodaway	June 12.	1881	3:45 p m.	SE.	Inverted cone.	495 to 825.
DeKalb	June 12.	1881	5:15 p. m.	ENE.	Funnel.	300 to 600.
Andrew	June 12.	1881	4:50 p. m.	ENE.	“	500 to 1,200.
Oregon	September —.	1881		NE.	“	
Shelby.	September 24	1881	3 p m	NE.		
Polk	September 26	1881	5:30 p. m.	NE.	Funnel.	
Nodaway	September 29.	1881	Night.	NE.	“	
Boone	September 29.	1881	Afternoon.	NE.	“	
Polk.	March 26.	1882	3 p m.	NE.		
Reynolds	April 8.	1882	6 p m.	NE.	Funnel.	300.
Saline	April 18.	1882	Afternoon.	NE.	Whirl.	
Henry and Saline	April 18.	1882	4:20 p. m.	NE.	Funnel.	600 to 5,280.
Henry	April 18.	1882	4:10 p m.	NE.		
Jackson	May 8.	1882	Afternoon.	NE.	Funnel.	
Warren	May 9.	1882	12:15 a. m.	NE.	“	
Montgomery, Warren and St. Charles	May 18.	1882	Afternoon.	ENE.	“	
Daviess	June —.	1882	3 p. m.	NE.	“	1,320
Gentry	June 12.	1882	5 p m.	NE.	“	
Cass	June 17.	1882	Afternoon.	NE.		40.
Cass	April 14.	1883	7:45 a. m.	NE.		50 to 70.
Greene	April 18.	1883	5:45 p m.	NE.	Funnel.	2,640.
Jasper	May 13.	1883	7:30 p. m.	NE	“	330 to 1,320.
Osage. Gasconade and Montgomery	“	1883	6 p. m.	E 40° N.	“	600.
Macon	“	1883	8:30 p. m.	E 12° N.	“	300 to 900.
Lincoln	“	1883	3 p m.	NE.	“	
Jackson	“	1883	8:30 p. m.	NE.	“	
Daviess	“	1883	5 p. m.	NE.	“	
Jasper	“	1883	9 p. m.	SE.	“	5,280.
Putnam	May 17.	1883	6 p m.	NE.	“	450.
St. Charles	May 18.	1883	9 p. m.	NE.	“	
Montgomery and Warren	“	1883	7 p m.	NE.	“	
Maries and Franklin	“	1883	Afternoon.	NE.	“	
Madison	“	1883	5 p m.	NE.		
Gasconade, Warren and St. Charles	“	1883	7:20 p. m.	NE.	Funnel.	240 to 1,320.
Howell	May 19.	1883	Afternoon.	NE.		
Livingston	May 20.	1883	5 p m.	E.	Funnel.	
Montgomery	June 8.	1883	Evening.	NE.		
Callaway	June 8.	1883	4 p m.	NE.	Turnip.	

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Livingston	June 12.	1883		NE.		
Scotland	June 15.	1883	8 p. m.	NE.	Funnel.	
Jackson	June 16.	1883	5:40 p. m.	E 10° N.	"	150 to 600
Monroe	June 17.	1883	8:30 p. m.	NE	"	
Montgomery and Warren	June 18.	1883	Afternoon.	ESE.	"	
Livingston and Caldwell	June 20.	1883	9:30 p. m.	E.	"	1,760 to 3,960
Carter	July 12.	1883	4 p. m.	SE.	"	
Gentry	July 13.	1883	Afternoon.	NE.	"	
Atchison	July 13.	1883	12:30 p. m.	E	"	
Shelby and Marion	July 13.	1883	Afternoon.	NE.	"	600.
Nodaway	July 13.	1883	12 m	SE.		
Macon	July 13.	1883	3:10 p. m.	NE.	Funnel.	
Lincoln	August 14	1883	6 p. m.	NE.	"	
	November 5.	1883	5 p. m.	E.		
Greene	November 5.	1883	2:15 p. m.	NE.	Funnel.	200 to 1,500.
Greene	February 11.	1884	4:30 p. m.	NE.	"	
Saline	March 11.	1884	3:30 p. m.	NE.	"	
Livingston	April 1.	1884	11 p. m.	ESE.	"	
Shannon	April 12.	1884	6 a. m.	NE.	"	
Gentry	April 26.	1884	6 p. m.	NE.	"	50.
LaFayette	June 8.	1884	Afternoon.	NE.	"	
Boone	September 23.	1884	5:30 p. m.	NE.	"	300.
LaFayette	April 1.	1885	8:30 p. m.	NE.	"	100.
Cass	April 1.	1885	12 m	NE.	"	
Cass	April 29.	1885	8:30 p. m.	ENE.	"	
Nodaway	June 2.	1885	7 p. m.	SE.	"	
Nodaway	June 3.	1885	6:45 p. m.	E.	"	
Nodaway	June 3.	1885	6:35 p. m.	E.	"	
Jackson	June 20.	1885	10 p. m.	NE.	"	1,200.
Howard	June 24.	1885	10:15 a. m.	NE.	"	
Greene	July 10.	1885	4 p. m.	SE.	"	
Nodaway	April 14.	1886	7:30 p. m.	NE.	"	300 to 1,320.
Holt	April 14.	1886	8 p. m.	NNE.	"	1,320.
Boone	April 14.	1886	Evening.	NE.		
Scott	April 14.	1886	Afternoon.	NE.	Funnel.	
Ray	May 9	1886	Evening.	NE.	"	1,320 to 2,640.
Pettis	May 11.	1886	11:30 a. m.	NE.	"	2,640.
Clay	May 11.	1886	11:05 a. m.	Easterly.	Heavy roll'g cloud	

TABLE II.—*Concluded.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
LaFayette	May 11.	1886	12 m.	NE.	....	1,200 to 5,280.
Jackson	July 8.	1886	5:30 p. m.	ESE.	Funnel.	....
Buchanan	August 17.	1886	6 p. m.	E	Funnel.	2,640 to 5,280.
Dallas	September 18.	1886	a. m.	NE.	....	....
Polk	September 18	1886	3:30 p m	NE.	Funnel.	2,640 to 6,600.
Jackson	May 13.	1887	10:30 p. m.	E.	"	Narrow.
Jackson	June 10.	1887	9:30 a m.	NE.	"	Narrow.
Mercer	June 20.	1887	5:15 p. m.	NE	"	Narrow.
Texas	February 19	1888	2:30 p. m.	N 62° E.	"	250 to 300.
Wayne	February 19.	1888	2 p m.	NE.	"	600.
Cass	May 6.	1888	4 p m.	NE.	....	Narrow.
Christian	July 4.	1888	3 p. m.	NE.	....	Narrow.
Carroll	August 6.	1888	2 a. m.	NE.	....	....
Carroll	August 5	1888	7:30 p. m.	NE.	Funnel.	Narrow.
Saline	August 6.	1888	1 a m.	NE.	....	....
Saline	August 5.	1888	10 p m.	Easterly.	....	Narrow.
Saline	August 5.	1888	9:30 p. m.	NE.	....	....
Vernon	August 6.	1888	11 p. m.	NE.	....	....

Table III.—*Relative frequency of Tornadoes by months and days, for Missouri.*

The index figures to the right and above the dates show how many times tornadoes occurred on that day of the month.

Month.	Day of Month.	No. of Days.	Total No. of Tornadoes per month.
February	11, $(19)^2$, $(23)^2$, and 27	4	6
March	10, 11, 26, 29, and 31	5	5
April	$(1)^2$, 8, $(12)^2$, $(14)^6$, $(18)^{16}$, 23, $(24)^2$, 26, and 29.	9	32
May	5, 6, 8, $(9)^3$, $(11)^3$, $(13)^8$, $(17)^2$, $(18)^8$, 19, 20, 24, 26, 28, $(29)^3$, $(30)^8$, and 31	16	44
June	1, 2 $(3)^3$, $(5)^2$, $(8)^3$, 10, $(12)^5$, 13, 14, 15, $(16)^2$, $(17)^2$, 18, $(20)^3$, $(21)^2$, 30, and (—)	17	31
July	2, 3, 4, 8, 10, 12, $(13)^5$, 16, 20, and 28.	10	14
August	$(5)^4$, $(6)^3$, 8, 14, 17, 19, and 29	7	11
September	$(18)^2$, 23, 24, 26, $(29)^2$, (—)	6	8
October	8 and 25	2	2
November	$(5)^2$, 8, and $(25)^3$	3	6
December	$(4)^5$, and 9	2	6
(—) Blank	$(—)^4$	1	4
Total		82	169

Note.—The blank (—) signifies date missing.

MEASUREMENT OF WIND VELOCITY.

RESULTS OF RECENT EXPERIMENTS.

By Professor C. F. Marvin.

Throughout all experiments heretofore made to determine anemometer constants and to develop a theory of their action, especially those of the Robinson type, a very important consideration seems to have been overlooked when using the instrument in the open air. In fact, some trouble has been taken by investigators to prevent the occurrence during their experiments of certain conditions that must always and unavoidably prevail when the anemometer is in actual use. It does not appear whether the oversight is a real one or the result of an erroneous assumption; in any case, the effect upon computed wind velocities is very considerable, amounting to as much as 10 per cent. and more, depending upon circumstances.

In the course of the many experiments that have been made upon whirling machines, every care has been taken to preserve a very regular and uniform rotation of the arm. The anemometer is thus made to pass with a steady motion through what is presumed to be still air, and its cups soon take up a rate of rotation corresponding to the velocity with which the anemometer as a whole advances through the air. In whirling machine experiments the advance of the anemometer being always along a circle arc, it is necessary to reduce to straight line motion, which is usually accomplished, approximately, if not rigorously, by taking the mean of results obtained for rotation of the whirling arm in both forward and backward directions. The ultimate result of investigations carried on in this way is some expression of the law, usually in the form of an equation, of the relation between the velocity of the centers of the cups and that of the advance of the anemometer through the air. That this law may be at all useful in practice it is necessary to assume that the result will be the same whether the anemometer is itself made to move through still air, or allowed to remain stationary in a wind of velocity equal to that previously given to the anemometer.

Presuming that all corrections for experimental conditions and disturbances have been properly made, and that the assumption just stated results in no sensible error, the final equation deduced from the experiments may be accepted as sufficient to enable one to compute wind velocities with reasonable accuracy, provided, of course, the velocities do not exceed greatly the limits within which the experiments were made, and bearing in mind, also, that the equation is for *very uniform winds.* Investigations generally close at this point, notwithstanding that a wind of even approximately uniform velocity never prevails in the open air. The violent and sudden changes between wide limits of the velocity of ordinary winds must be apparent to every one, and it is hardly to be supposed so important a point has been overlooked. Recognizing that the revolving parts of an anemometer exposed to a variable wind, will, in consequence of their inertia, fall short of their proper rate of revolution when the velocity of the wind is increasing, and then catch up and run too fast when the wind velocity diminishes, it is quite probable it has been generally assumed, that the amount lost in the one case is just about balanced by that gained in the other, so that the mean rate of revolution is not affected. Such, however, is not the case, from the fact that the resultant force acting on the cups when the velocity of the wind is increasing and tending to increase the velocity of the cups, is much greater than the corresponding force in action when the wind is diminishing in intensity, and which tends to retard the velocity of the cups. In one case it is a question of the resistance of the air to the concave side of the hemispherical cups, and in the other that to the convex surface; the latter being smaller, the cups will always continue to move more rapidly and longer after the wind has diminished in velocity, than they lagged behind when the wind was increasing, so that, *the mean velocity of the cups exposed to a variable wind, is appreciably higher than it would have been had the wind been quite uniform, and of a velocity equal to its mean velocity.* By variable winds in this connection is meant winds whose velocity changes considerably within a few seconds; in fact, any ordinary wind in the open air. It will

therefore follow, from the above, that if the equation of an anemometer, whose constants have been determined upon a whirling machine, be used to reduce observations made in the open air, the computed wind velocities will be too high, by an amount which will depend upon the moment of inertia of the cups and revolving parts, and the nature and extent of the variations of the wind. Since these latter are quite beyond determination, at least are too complicated to be introduced in a practical formula, it is quite impossible with anemometers of the Robinson type, to make a very accurate measurement of the mean velocity of a variable wind, unless the moment of inertia of the cups is very small, relative to the wind pressures, and even then the result is still affected by another error, to which most anemometers are subject, and that is also the result of variations in the wind velocity. This will appear from the following discussion:

A straight line equation of the form,

$$V = a + bv;$$

where V = velocity of the wind and v = velocity of the cup centers, a and b being the constants, whose values are determined from the experiments; has often been used as representing the anemometer law, but recent experiments upon a very large whirling machine, and with an instrument having cups 4 inches in diameter on arms 6.7 inches long have shown that such an equation is insufficient over a range of velocities such as commonly occur, and if greater accuracy than within ten per cent. is desired, it is necessary to introduce a third term involving the square of the velocity in the second member of the equation, which then takes the form

$$V = a + bv - cv^2.$$

This equation is purely empirical and simply happens to fit the experiments, so that it is of little value for velocities beyond the limits of the experiments.

The equation for perfectly uniform winds of the above described anemometer, which is that of the Signal Service, is as follows:

$$V = .225 + 3.143\ v - .0362\ v^2,$$

V and v being in miles per hour. The computed values, with one exception, agree with the values found in the experiments to within less than one per cent.; the highest velocity, however, being a trifle less than 30 miles (50 kilometers) per hour.

When the anemometer is used in the open air, the quantity which is observed is the mean velocity of the centers of the cups, and if this is variable it may be represented by

$$V_m = \frac{(v_1 + v_2 + v_3 + \cdots v_n)}{n}.$$

It is evident, however, that $(v_m)^2$ is not the expression which should appear in the third term, but one having the form

$$\frac{v_1^2 + v_2^2 + v_3^2 + \cdots v_n^2}{n}.$$

The variations of v are between such wide limits, under even ordinary circumstances, that the difference between the square of the mean velocity and the mean of the squares is sufficient, notwithstanding the small value of the coefficient of v^2, to introduce an error amounting to over one per cent. in the value of V. This is not so serious but that it might be overlooked or corrected in ordinary practice, but this inertia effect is too large to be so easily disposed of, and it therefore seems necessary, if accurate results are to be obtained, to materially alter the construction of the Robinson anemometers heretofore so largely used in measuring wind velocities. It also follows that ordinary anemometer constants, determined by whirling machine experiments, are largely incorrect for open air conditions.

In two or three instances, anemometer investigations have been supplemented by subsequent comparison in the open air, but generally there has not been sufficient difference in the inertia of the various cups to produce a large influence upon the relations between the two anemometers under comparison, both of which undoubtedly gave too high a wind velocity, but may not have differed much from each other, and the attention of the investigator seems not to been directed to this consideration.

When the anemometer experiments of the Signal Service were first taken up, the whirling machine was placed in the

north court of the War Department building. This, though practically closed on the sides, was not roofed over, and it was quickly found the results were being seriously influenced by the small air-currents that circulated about the court, the tendency being for the cups to run too fast, as it were. The true explanation of this was not discovered at first, but it was undoubtedly due to the variable character of the wind causing the cups to take up a higher rate of revolution than they should. Within the last year whirling machine experiments have been carried on in the open air in England, by Messrs. Whipple and Dines, and their results show every evidence of the influence of air movement over and above that due to the whirling arm.

The foregoing discussion leaves little doubt as to the correctness, in general, of the conclusions stated above, but abundant confirmation is afforded in a variety of experiments which have been made recently.

During the progress of the whirling machine experiments referred to above, in which the constants of the Signal Service anemometer, among others, were determined, it was found desirable to make use of a very small anemometer whose constants, for uniform motion, could be determined with great precision. This instrument was made with great care so as to have the least possible friction, and yet to be of the same general style as the larger anemometers. The cross arms and cups which form the important parts of the instrument were extremely light; weighing a little more than two grams or a trifle less than half as much as a nickel five cent piece. The cups were made of a fine dense quality of writing paper which was carefully formed into a cone open at the base, the angle at the vertex being just 60°. Excellent cones of this kind can be made by folding up semi-circular pieces of paper, having provided a little extra along the diameter for lap, at which place the parts are pasted together. These cones were fastened to the pieces of fine knitting needles, which served as arms, by means of wax, much in the same manner that the large metal cups are secured to the steel arm by use of solder. The spindle was correspondingly delicate and made of hardened steel in the most approved man-

ner, being provided with a fine worm or screw at the lower end which engaged in the teeth of a rather large worm wheel of skeleton construction so as to be very light and moving with great freedom on a small axis.

The dimensions of this set of cups and arms were:

Length of arms from axis to centers of cups (axis of cones)	1.47 inches.
Diameter of cups at mouth or base of cone........................	1.25 inches.
Weight of arms and cups complete..........	2.45 grms.

In addition to the sets of cups just described a precisely similar set was used in which the cups were composed of aluminum, the thickness of which happened to be just the same as that of the paper employed in the other cups, viz., .005 inch. It may be interesting to note that this set of cups and arms was nearly twice as heavy as the paper set.

The constants of this anemometer on account of its small size could be determined much more satisfactorily than was possible in the case of larger ones, the arm of the whirling machine in some of the experiments being 28 feet long and in others 35 feet. None of the evils that always attend whirling machine experiments upon ordinary anemometers, such as mitwind and abnormal friction due to centrifugal effect, were at all appreciable in this case. Experiments were repeatedly made in less than two minutes, and show the closest agreement with each other. In this short time no appreciable amount of mitwind was developed, the small anemometer being nearly 10 feet above the whirling arm, and connected thereto by a tapered rod of about $\frac{5}{8}$ inch diameter at the largest part.

Each experiment was made in two parts, a result being obtained when the whirler was revolved in a right handed direction, and another for rotation in the opposite direction; the mean of these two being taken for the final value and considered as the result that would have been obtained had the motion been in a straight line instead of circle arcs. Some idea of the smallness of the errors of observation may be gained when it is stated that throughout all the experiments, of which about fifty were made, the difference between the results for right and left hand rotation of the whirler was preserved with great uni-

formity, notwithstanding that this difference was very small, being proportional to the ratio of the anemometer arm ($1\frac{1}{2}$) to the whirling arm (470), a difference so small as to be easily lost in errors of observation, if these were large.

The experiments, without being corrected or altered in the least from the observed values, are combined in the following equations:

For paper cups,

$$V = .325 + 3.170\, v - .0185\, v^2$$

For metal cups,

$$V = .374 + 3.157\, v - .0158\, v^2$$

The largest difference between the computed and observed values was, for the first equation 0.8 per cent., and for the second only 0.5 per cent., the mean error being much smaller.

The extreme lightness of the cups of these anemometers makes it quite probable that, even in the most variable wind, the mean velocity of the cups is very little greater than it would be for a uniform wind. In other words, the inertia effects as discussed above, are nearly or quite inappreciable in this case. Moreover, the error due to the use of the square of the mean velocity instead of the mean of the squares, is scarcely half as great as in the case of the regular Signal Service anemometer, so that it is believed the actual wind movement is measured within one per cent. by the small anemometer.

Open air comparisons between this and the larger anemometer have been made by exposing them side by side on a high pole erected on the instrument platform on the roof of the Signal Office. The small anemometer was carried on a rather slender but perfectly rigid arm, extending horizontally from the larger support for the heavier instrument. The distance between them being nearly three feet, the smaller being two or three inches higher than the larger. The comparisons generally lasted two or three hours at a time, during which the connecting arm between the two instruments was kept about at right angles to the wind, so that both were equally exposed and each out of the influence of the other. Electrical registration was effected by the usual break circuit and chronograph method.

the devices being so arranged that record was made independently by each anemometer for a wind movement of about one-tenth mile. In this way the velocities of the two instruments could be accurately measured for very short intervals, and some idea of the amount and character of the variation ascertained.

Two methods of study were followed: first, to compare the mean velocities over a half hour or more; second, to select individual velocities, that were nearly the same, and compare with the corresponding velocities recorded by the other anemometer at the same time. In this way a large range of velocities, from the lowest to the highest, could be obtained, among which, if the inertia theory is correct, one would expect to find the low velocities largely discrepant and closer and closer agreement as the maximum was approached. Table I, gives the results of a few experiments, reduced by the first method, showing the differences between the mean velocities. The duration of the experiments, except 9 and 10, being from 15 to 25 minutes, Nos. 9 and 10 being about an hour and a half.

TABLE I.

No. of Experiment	Vel. miles per h'r. Small Anemo. Metal Cups.	Velocity by S. S. Anemometer	Difference.	Per Cent.
1	22 60	25.20	+ 2.40	10 6
2	17.09	18.78	+ 1.69	9.9
3	20.22	21.94	+ 1 72	8.3
4	21.64	23.69	+ 2.05	9.2
5	22.05	24.16	+ 2.11	9.6
6	20.43	22.49	+ 2.06	10.1
7	21 25	23.17	+ 1 92	9.0
8	18.98	20.86	+ 1.88	9 9
9	13.70	15.01	+ 1.31	9.6
10	11.22	12.26	+ 1.04	9.3

Experiment No. 9, which was of longer duration than those preceding, was divided into nine portions, starting with all the low values and so on up to the highest, grouping the various values between arbitrary limits. The results are shown in

TABLE II.

	Miles per Hour.								
Velocity by Signal Service Anemo....	8.946	9.95	10.91	11.52	12.67	14.39	15.89	17.78	20.34
Velocity by Small Anemo................	6.718	8.55	9.49	10.23	11.31	12.81	14.53	16.95	19.57
Difference, per cent.	*33.	16.2	15.	13.	12.	12.	9.	5.	4.

* Only a single observation in this case.

The results in Table III were obtained from experiment No. 10 by taking groups of eight or ten values that were nearly the same, and taking the mean.

TABLE III.

Anemo.	Velocity in miles per hour.			
Signal Service...........	7.349	9.253	12.16	19.40
Small........................	6.478	8.259	11.29	18.26
Difference..................	13.5	12.	7.7	6.2

One more set made up from experiment No. 1 will show still further the effect of the inertia of the cups.

TABLE IV.

Anemo.	Velocities in miles per hour.		
Signal Service...........	23.77	28.54	31.63
Small...................... ...	21.10	26.67	30.8
Difference, per cent....	12.7	7.	5.2

From the results in Table I, it appears the mean velocity of the wind as determined by the use of the Signal Service anemometer is, in most cases, about ten per cent. too high. Tables II, III and IV, show the distribution of the gain, those above the average velocity being less in excess than those below. The amount of difference between the two anemometer, does not seem to be at all dependent upon the actual velocities, but upon the relation the latter bears to the mean velocity of the experiment. This is shown by comparing the difference at a certain velocity in one experiment in which the velocity under consider-

ation is above the average, with the difference in another experiment in which the velocity is below the average, the mean velocities being very different in the two cases. Thus at 19.6 miles per hour in table II, S. S. is only fourteen per cent. too great, while at 21.1 miles in Table IV, S. S. is nearly thirteen per cent. too great.

If these results are due to the inertia of the cups, taken in conjunction with the different effects of the wind in action upon the front or concave sides, as compared with that upon the back or convex sides, it is reasonable to suppose that the Signal Service cups could be made to gain still more by increasing their weight. (It was not practicable to lessen the weight of the cups). For this purpose four circular plates of moderately thick sheet lead were hammered into form to just fit the inside surfaces of the anemometer cups, but which were only partially covered thereby, and to which the pieces of lead were securely fastened. The moment of inertia of the cups was greatly increased, but with no other appreciable effect, unless to increase slightly the pivot friction. Results obtained during the whirling machine experiments have shown that such an increase in the friction can be neglected without the least error.

Comparison of the anemometers with weighted cups gave results as follows:

TABLE V.

Velocities by small anemometer miles per hour.

		S. S. Higher		S. S. Higher		S. S. Higher		S. S. Higher.
Mean Velocity.....	10.38	15.2%	13.68	13%	12.60	12%	4.86	7.6%
Selected Velocities................	15 69	4 5%	24.06	2 6%	20.64	5%	6 46	−0.6%
	9.42	13 5%	11.79	17 6%	10.76	12%	5 02	+8 8%
	7 45	16.1%	8.18	22.9%	7.22	23%	4.10	+7.5%
	5 8	17.4%	5.98	18. %				

The increase in the percentage differences going downward in any vertical column shows how much more the anemometer gains during velocities less than the average than for higher rates.

These results show conclusively the necessity of making anemometer cups of extreme lightness if any reasonable accuracy is to to be attained in wind measurement. The variations in the velocity of the wind are not necessarily the same from day to day, and it is therefore impracticable to accurately determine constants for heavy cups.

As one other line of investigation promised to give additional information, a few experiments were tried. These consisted in setting the cups of the anemometer whirling in still air at a certain velocity, and noting the time they would continue to revolve before they came to rest. When the direction of rotation was such that the back or convex surfaces of the cups advanced in the air, the energy of rotation was absorbed much more slowly than when the concave surfaces were made to advance. This is shown in the following table:

TABLE VI.

	Cups Without Weight.	Cups Weighted
Rotation forward	1 min. 39 seconds.	5 min. 11 seconds.
" backward	1 " 16 "	4 " 11 "
Percentage 1st greater than 2nd	30 %	24 %

The initial velocity of the cups was about the same as would occur in a wind of about ten to fifteen miles per hour, the results above being the mean of several observations in each case. The small anemometer was not tested in this way, as it was feared serious damage might result. However, when made to spin, as fast as possible by blowing upon it, the cups would come to rest in about twenty seconds.

As was stated, the wind may at one time blow more steadily than at others, and in such cases the anemometer will require a different equation for each condition. On this account a large number of comparisons under a great variety of circumstances are necessary before an approximate equation for the ordinary Signal Service anemometer can be determined. The values given above are probably not very far out, but the range of velocities is not sufficient.

In the open air comparisons the small anemometer has been taken as a standard, because its constants have doubtless been determined with less error and under circumstances that did not necessitate the application of uncertain corrections to the observations, and the extreme lightness of its cups make it more suited to a variable wind.

Dubinsky has recently published in *Rep. für Met.*, Band XI, No. 7, the constants of two small anemometers quite similar in dimensions to the one used in these experiments. The results do not differ widely from those given above, but it is impossible under the circumstances to make a close comparison. The cups of the two anemometers were about 1.59 inch in diameter. The arms to the centers of the cups were a trifle less than 2 0 inches long. The anemometers were tested in both St. Petersburg and at Hamburg, upon whirling machines with arms about 11 feet long. The measurement of "mitwind" in these tests is believed to be considerably in error, as was pointed out in an article in *Science*, Nov. 23d, 1888. Regret was expressed at that time that the investigator had not published the necessary data from which the linear velocity of the cups could be computed, but this was unnecessary and due to an oversight, as on further examination it is found that electrical registration is effected in the anemometer after 1,000 revolutions. Using this value and the length of the arms the equations are made to correspond in form to those already given. The constants are also converted into units for miles per hour instead of kilometers per hour. For the two anemometers without correcting for mitwind the equations are, as tested at St. Petersburg:

No. 74. $V = .644 + 3.166\, v - .00281\, v^2$,

No. 75. $V = .565 + 3.277\, v - .00773\, v^2$,

Signal Service small. $V = .374 + 3.157\, v - .0158\, v^2$.

It would be interesting to know how the cups of the European instruments compare in weight with the Signal Service instrument, but these data are not given. The constant term in the first two equations is noticeably large, and the amount of curvature as shown by the coefficient of the third term is comparatively small. This feature has been noticed before when comparing

results, upon larger anemometers, obtained by Dohrandt, who has also done a great deal of work upon the St. Petersburg whirling machine. A possible explanation of this may be found in the fact that the centrifugal effect when using so short a whirling arm is very great, especially at high velocities, in consequence of which the abnormal friction seriously retards the revolution of the cups, so that the higher velocities are too low and fall more nearly than they should into line with the lower velocities. One anemometer, not very different in dimensions from the Signal Service standard, was found to have the following equation.

$$\text{VIII.} \quad V = .60 + 2.631\,v - .00699\,v^2$$
$$\text{Signal Service.} \quad V = .225 + 3.143\,v - .0362\,v^2$$

The difference between the two is greater than can be attributed to the difference in dimensions, and may possibly be due to the use of a wrong correction for "mitwind," which in the St. Petersburg experiments was five per cent. The first term or friction factor in Dohrandt's, as also in Dubinsky's, equations, are considerably larger than the corresponding values in Signal Service equations, and at the same time the remaining coefficients in consequence of their smaller value imply that the linera velocity of the cups of Dohrandt's anemometer would when exposed to the same wind be relatively faster than that of the Signal Service instrument. This seems hardly consistent with the larger friction terms, and needs further explanation.

If the velocity of the whirling machine was irregular in Dohrandt's experiments, the tendency would be to make the anemometer run too fast, or the same result might happen from fluctuations in the mitwind.

It is intended to extend the open air comparisons so as to obtain sufficient data to establish an equation for the heavier anemometers when used in variable winds.

Considerable uncertainty must always exist in regard to the anemometer problem until some method is devised for determining the instrumental constants without the necessity of substituting motion of the anemometer for motion of the air. It is not improbable that an appreciable difference may result from these different conditions.

SIGNAL OFFICE, WASHINGTON, D. C., March 14, 1889.

MAGNETIC PHENOMENA IN THE SOUTHERN HEMISPHERE.

By Professor Richard Owen.

The object of the present paper is to give some particulars regarding certain magnetic experiments made in the Southern Hemisphere, at Melbourne, Australia, as compared with results obtained in the northern half globe.

To avoid confusion, or misunderstanding, I propose, in this paper, when designating one or other end of a magnetic needle to employ the term *plus* for the so-called north or boreal, or marked or north-seeking, end, and will apply the term *minus* to the unmarked or south-seeking end; also I will denominate as *plus or positive magnetism* the phenomena exhibited at the north-seeking end, and *vice versa;* using phraseology similar to that often applied to electricity. This does not necessarily imply that there is more molecular motion at one end than at the other; although the fact of dissimilar magnetisms and electricity attracting, while similarly magnetized or electrified bodies repel each other, seems to render the supposition not improbable that the attractive impulse is due to a law of equalization or a tendency to equilibrium.

Before comparing the results obtained in the two hemispheres, some preliminary remarks on the general principles of magnetic phenomena may not be out of place; giving the explanations somewhat of a popular form, and citing experiments which can readily be repeated.

LAW DISCOVERED BY OERSTED.

1. Let the operator place to the N. of him a coil, six or eight inches in diameter, of No. 22 insulated copper wire, upon the upper arm of a wooden filter stand, with a single cell of (say) bichromate battery S. of the coil, the vertical plane of which latter stands E. and W. Placing the magnetic needle on the lower arm of the stand within the coil and in the same vertical plane, then clamping to the positive pole that end of the coil which winds from E. up and over to W., while we clamp the end of the wire which thence descends to the negative pole of the

battery, then the needle, oscillating within the coil, as soon as the zinc is lowered into the liquid, will be reversed. The law, that Oersted discovered, shows the needle setting itself at right angles to the current of electricity; and the statement is usually added that the direction to which a given end of the needle will point depends upon whether the needle is *above* or *below* the current. This is not strictly correct, because the needle in the above experiment will be found constantly to point with its plus or marked end to the N., when carried any where near the *outside* of the coil, as well below as above; whereas any where *inside* of the coil, the plus end will point S.

If we regard the plane of the coil as the magnetic equator and its zenith as the meridian of land centre (middle Europe) then by a slight movement of the coil's plane, as it pivots on the wooden arm at the zenith, the needle is made to move responsively; and may thus be caused to point to the meridian of central N. America at Boothia Felix, or again to the region of northern Siberia, near the meridian of central Asia; indicating the probable secular movement of the horizontal needle, as detailed in a paper published in the Proceedings of the Montreal Meeting of the A. A. A. S. (Vol. II, p. 336.)

DECLINATION.

2. Instead of forming the above coil, let us wind the wire over a grooved wooden globe (such as described in the October, 1886, No. of this journal, and figured in the St. Louis *Valley Naturalist* of October, 1878), commencing about Lat. 70° N. and coiling from E. over to W., finishing at Lat. 70° S. Carrying a small magnetic needle round this globe, we obtain results similar to those with the coil, and can demonstrate the variation or declination of the compass, such as pointing with the plus end due S. when carried somewhat N. of Boothia F., or with the minus end due N. on passing the magnetic pole in Victoria Land; or 90° due W. in part of Greenland, etc. The agonic (or line of no variation in W. Hem.) can also be traced by carrying the needle from over that part of the wooden globe representing Rio de Janeiro, the mouth of the Amazon, the region of W. Indies, N. Carolina, Cleveland, O., Hudson's Bay to Boothia

Felix; as, also, on the under side of the globe, the agonic through Australia and the Caspian Sea, etc.

EARTH CURRENTS.

3. By driving, at different points of the compass (as also detailed in the above No. of this journal), iron rods 6–8 feet into the ground in a middle lat. of the northern hemisphere, and conveying a wire from one of these at a time through a window to a table on which stands a delicate galvanometer, while the other wire communicates with a metal tank, or some metallic reservoir under ground, we obtain strong currents of electricity, such as I described in the N. Y. semi-weekly *Tribune* of May 28, 1878, showing the resultant currents for Indiana to be from ESE. to WNW., causing the needle to point throughout that State with a variation at present of about 5° to 6° E.

CONNECTION BETWEEN SEISMOLOGY AND TERRESTRIAL MAGNETISM.

In the same No. of this journal, mention is made of a communication to the N. Y. *Tribune*, in which attention was called to the fact that during an earthquake at Caraccas, the wire extending from the SE. rod, northerly to the reservoir, when clamped to the galvanometer, caused the needle to oscillate violently and continuously, instead of settling as usual in a few minutes. The same kind of magnetic disturbance was exhibited from 24 to 48 hours before an atmospheric storm, when the needle that usually indicated for certain underground currents a deflection of 6° to 7°, increased its amplitude to 35° or 36°. With one wire brought from the lightning rod, while the other communicated with any of the underground rods, the current was invariably from the atmosphere to the earth; hence the variations in terrestrial magnetism, especially during thunderstorms.

THERMO-ELECTRICITY.

4. The experiment showing the conversion of heat into electricity, when the conductivity of the two transmitting metals differs greatly, was detailed in this journal, Vol. III, No. 8, Dec., 1886, p. 352, and, therefore, need not here be repeated, except for reference when we come to recall one of the causes which

may give rise to thermo-electrical currents in our earth's crust. The atmo-thermal maxima and minima do not, however, penetrate in middle latitudes, such as Paris, to the invariable stratum (for that lat. about 92 feet), and there register the geo-thermal maxima and minima, until about six months, after those periods on the surface, according to the statement of Humboldt and others.

MAGNETIC INDUCTION AND VERTICAL POLARITY IN NORTHERN HEMISPHERE.

5. Attention was called in Dr. I. C. Draper's "Year Book" for 1872, at p. 40, to my experiments, showing horizontal as well as vertical polarity in the T rails of a railroad. The same phenomena may be readily exhibited by applying a magnetic needle to the lower part of a stove, tinned bucket, long hinge and the like, in a vertical position, when the minus end of the needle will be attracted, while at the upper end of those iron articles the plus end will be attracted. With care, a middle zone about half way between top and bottom will be found neutral, the needle there assuming a N. and S. direction, the earth reasserting its power, which is overcome at top or bottom by the influence of the stove, etc., sufficiently to deflect the needle 90°, and make it stand E. and W. *Unlike* magnetisms attract and *like* repel, hence our hemisphere attracts to the *bottom* of the stove, etc., that magnetism which will cause the minus end of the needle to present itself, but repels to the *top* of the stove magnetism similar to its own; consequently the plus end of the needle is attracted just as it is by our *northern* earth.

Evidently, if the above explanation of these phenomena is the correct one, the action of the needle, when it is applied to the stoves, etc., in the *southern* hemisphere, would be similar, with *reversed* signs.

I had endeavored by letter to induce the Astronomer Royal at Cape Town to repeat those experiments; but he either did not receive my communication or failed to answer it. It was, therefore, with great satisfaction I learned that Prof. F. M. Webster, of Purdue University (a member of the Agri. Exper. Station), had been detailed for scientific purposes to Australia.

He had previously visited New Harmony on official business, and had taken an interest in the experiments detailed in No. 5. At my request, he readily consented to repeat them in the southern hemisphere; and recently very kindly wrote me from Melbourne, Australia. In his letter, dated January 22, 1889 (from which I feel assured he would permit me to publish extracts), he says: "The result of my experiment with the magnetic needle on this side of the equator * * * turns out exactly as you anticipated it would. The upper end of an iron bar, or any other iron object, *rejects* the marked end of the needle, and the lower portion *attracts* it. The workings of the needle are very marked here, and I shall try it in the vicinity of the equator, on both sides, on my way home."

This was of course to me, as I hope it will be to others, very interesting and satisfactory.

The behavior of the needle, when applied in regions near the equator, to a vertical iron bar, etc., I should expect to be similar to that at the neutral zone of the stove, etc., namely, it seems probable that the needle, whether applied at the top or bottom of the iron vessel, would continue to place itself in the magnetic meridian of the places, the vessel not evincing any polarity, or difference in attraction, because the inductive power of one hemisphere would be equally balanced by that of the other hemisphere. This is, however, under the supposition that Prof. Webster crosses the earth's equator, where it corresponds pretty nearly with the magnetic equator of dip, as well as the magnetic equator of intensity, which is approximately the case on the direct route from Australia to San Francisco.

THERMO-ELECTRIC MAGNETISM.

Our earth revolving from W. to E. and meeting a noon-day sun on any given latitude, while about 6,000 miles W., on the same latitude the sun was only rising, would tend to produce currents of electricity from E. to W.; but modified by currents from the tropics to the poles, as well as by changes from ocean to earth, and from rock to soil, etc., on the principle exhibited by the experiment alluded to in No. 4. These thermo-electric

currents thus seem to have considerable influence in determining the declination of the magnetic needle.

TELLURIC MAGNETISM BY SOLAR INDUCTION.

As, however, the average *rotation* period of relatively persistent spots on the sun corresponds with the *magnetic* period of about 26½ days, and that again with an observed similar *auroral* period; and as, furthermore, solar storms are instantaneously felt all over our earth, and perihelion increases magnetic intensity, and auroral displays affect the magnetic needle synchronously at all observing stations, it seems probable that the earth's magnetism is derived chiefly by direct induction from the sun, although also modified by changes in that orb. What is induction, if not conduction (or motion transmitted) through a rare medium, such as our atmosphere, or cosmic ether, whether interstellar or intermolecular?

The transmission of magnetic motion seems facilitated by the favorable arrangement of particles or molecules. Thus when a short bar of iron or steel is suspended between the poles of a powerful electro-magnet, it sets *axially*, the elongated mass and elongated particles pointing with their plus and minus ends from pole to pole. But apply crushing power to the ends of this bar, then said ends will set *equatorially*. This, in connection with the magne-crystallic axis, may explain the frangibility of iron bridges exposed to frequent vibrations, as well as the brittleness of Prince Rupert drops, or glass too quickly cooled, or the rejection, by railroad companies, of such railroad iron as has been carelessly unloaded, tending to disarrange the polarity of molecules, whereby the cohesive strength is greatly diminished.

SUGGESTIVE HYPOTHESIS.

All phenomena and materials of the Physical Universe, observable by means of our senses, may be defined as matter in motion. This of course excludes spirit or mind cognizable by the imagination. Motion in matter, whether mass or molecular, visible or invisible, has been transmitted to our earth directly or indirectly, most probably through the so-called luminiferous or cosmic ether, chiefly from our sun, aided by other solar orbs,

less powerful in their influence on our earth because of their greater distance. Of course any planetary or lunar effect is derived indirectly from the sun.

Scientists now generally admit that these various modes of motion (such as light, heat, electricity, magnetism, chemical action, mechanical power, vital and nervous energy) are convertible and indestructible. W. Kedzie, in a work on "Solar Heat, Gravitation and Sun Spots," has shown the probability that gravitation is transmitted in right lines, through the attenuated ether, and that attraction (citing Newton as authority) is, in no sense, a pull, but rather a *vis a tergo:* in other words that all the heavenly bodies are constantly receiving (from all other heavenly bodies) motion, which they again transmit to all other heavenly bodies, hence these solar orbs are sources of light and heat without any necessarily destructive combustion, even under such thermal increment as may convert metals (which on our earth are usually solids) into vapor.

Sir Wm. Thompson, in a recent address (see *Science* for March 1, 1889), expresses the conviction that a *mechanical* solution of electrical phenomena will be found.

As a suggestive contribution toward that desired end, let us suppose gravitation a motion in direct or right lines through interstellar space. On approaching our atmosphere, it seems not unreasonable to conceive that a portion of direct motion might be converted, by resistance, into undulatory light, which again when obstructed by a non-diaphonous body is partly converted into radiating vibratory heat-motion; that again, as in experiment No. 4, into electricity, which may assume (somewhat as suggested by Ampère) more of a vertical or spiral movement, in the atmospheric magnetic field, as well as in the molecular ether, so that when thus coiling around particles or molecules of oxygen, iron, nickel and the like, these molecules place themselves paramagnetically and at right angles to the plane of the electrical current.

TENDENCY TO EQUILIBRIUM.

The law of gravitation is that every particle of every body attracts every particle of every other body, and the force is

increased geometrically by diminished distance. Each particle is endowed with mobility, and that mobility may be increased or diminished by transmission of motion from particle to particle.

A natural consequence of inequality of particles, in a body, implies an inequality in motion; and the law or tendency seems to be toward the restoration of equality or equilibrium, by bringing the fewer particles (whether because that body is smaller or lighter) of the smaller or less dense body, by so-called attraction, toward a larger or denser body. If the bodies or particles, from any cause, are prevented from approaching, there may be merely a transference of motion from one body to restore the equilibrium in another. The differentiation of the two modes of motion, heat and electricity, causes the winds, storms, and other meteorological changes in our atmosphere, and gives rise to currents in the ocean and in the earth's crust. Difference in the increments of heat and magnetism explains the phenomena of magnetic declination, inclination, intensity, and diurnal solar, as well as, probably, secular variation.

As we find by experiment a constant transference of electricity from the atmosphere to the earth, it seems not unreasonable to infer that our atmosphere is constantly receiving, from the sun and other solar orbs, through the interstellar attenuated medium, fresh increments of motion, either as gravitation, light, heat, or electricity, certainly as motion in some form, so essential to the existence of our planet.

NEW HARMONY, INDIANA, March 15, 1889.

P. S.—Since the above communication was sent, Prof. Webster has returned to the United States, and kindly writes me, under date of March 25, 1889, the results of his magnetic experiments at and near the *terrestrial* equator at about longitude 165° west. When, according to the captain's reckoning, they were crossing the equator at 3 P. M., the magnetic needle applied to iron articles showed the same phenomena as in the southern hemisphere, and again when tested at 7 P. M., same day, at which time the vessel was estimated to be 40 miles north

of the terrestrial equator. But at 9 P. M. *the signs were reversed*, showing the phenomena, as in the northern hemisphere; estimated distance north of equator, 75 miles. This agrees well with the diagrams in the 1870 edition of Snell's Olmsted, exhibiting the equators of dip and intensity as intersecting the terrestial equator, respectively, at about 100° and 150° west longitude, *and thence running slightly north of west.* Perhaps at 8 P. M., when, say 50 miles north of the terrestrial equator, the neutral zone might have been found.

In a similar manner, an intelligent traveller, by providing himself with a pocket compass, and a short iron rod, or tinned iron bucket, could determine, approximately, the *magnetic* equator, while traversing land or water, in equatorial regions.

R. O.

NEW HARMONY, IND., March 27, 1889.

EDITORIAL NOTE.

PROFESSOR W. J. HERDMAN, M. D.—It gives us great pleasure to announce that Professor Herdman, of the University of Michigan at Ann Arbor, has joined the editorial corps of this JOURNAL, to begin with the first number of Volume VI. Prof. Herdman will have more directly in his charge the division of medical climatology. We expect that under his supervision this highly important division of meteorology will find in this JOURNAL its proper proportion of attention. We desire that literature relating to medical climatology, and intended for notice or review, be sent directly to Dr. Herdman.

CURRENT NOTES.

KANSAS WEATHER SERVICE.—A reprint from the Sixth Biennial Report of the Kansas State Board of Agriculture, contains the report of the State Meteorologist and gives us an idea of the great meteorological activity in that State. Sergeant T. B. Jennings is indefatigable in extending the service, collecting all earlier observations, publishing frequent reports, and studying

the special phenomena of Kansas. Professor J. T. Lovewell gives temperature and precipitation tables for the state for a series of years, probably including all available and reliable observations. Sergeant Jennings has made a special study of the notable hot winds of Kansas during the year, and finds their source in the elevated plateau of southeastern New Mexico. We should think that a study of their relations to the general weather conditions would be profitable. Professor Lovewell finds no confirmation of the theory that Kansas is the peculiar home of tornadoes. In the last two years he found but one well-marked tornado in the state.

IRRIGATION SURVEY OF THE ARID LANDS.—The Sundry Civil Bill passed by Congress last August, appropriating for the fiscal year ending June 30, 1889, contained the following important item:

"For the purpose of investigating the extent to which the arid region of the United States can be redeemed by irrigation, and the segregation of the irrigable land in such arid region, and for the selection of sites for reservoirs and other hydraulic works necessary for the storage and utilization of water for irrigation, and the prevention of floods and overflows, and to make the necessary maps, including the pay of employés in field and office, the cost of all instruments, apparatus and materials, and all other necessary expenses connected therewith, the work to be performed by the Geological Survey under the direction of the Secretary of the Interior, the sum of one hundred thousand dollars, or so much thereof as may be necessary.

And the Director of the Geological Survey, under the supervision of the Secretary of the Interior, shall make a report to Congress on the first Monday in December of each year, showing in detail how the said money has been expended.

All the lands which may hereafter be designated or selected by such United States Surveys for sites for reservoirs, ditches, or canals for irrigation purposes, and all the lands made susceptible of irrigation by such reservoirs, ditches, or canals, are from this time henceforth hereby reserved from sale as the property of the United States, and shall not be subject, after the passage of this act, to entry, settlement, or occupation, until further provided by law: Provided, the President may at any time in his discretion, by proclamation, open any portion or all of the lands reserved by this provision to settlement under the homestead laws."

The first report of Major Powell under this appropriation was duly made in December last, and is printed as Senate Ex. Doc. No. 43, 50th Congress, 2nd Session. Although but a short

time had then elapsed since the passage of the act, this report shows that much topographic work had been done in the arid lands during the fall of 1888, in addition to that previously completed, and that preparations are being made for its rapid extension during the coming year.

In this field, under the direction of Mr. Henry Gannett, Chief Topographer, the Survey has developed a staff of well trained topographers with highly systematized methods of operation in which the means are adapted to the ends with an efficiency and economy too seldom found in Government work. In addition to prosecuting the topography of the arid lands for which the survey is thus found in readiness, Major Powell has organized a new division of irrigation and hydrography under the direction of the eminent physical geographer and physicist, Capt. C. E. Dutton.

To this division is given the investigation of the scope, methods, extent, history, and legal aspects of irrigation, and of the special questions of hydrography, climate, physical geography, geology, agriculture and engineering pertaining thereto,—the complement of which is aptly expressed by the term *irrigation survey of the arid lands.*

The advance party of this division, consisting principally of hydrographic engineers with younger assistants, has occupied a camp on the Rio Grande in Northern New Mexico during the entire winter, for the purpose of adapting intruments and methods to meet the peculiar difficulties of hydrographic work on the rapid mountain torrents of the west. Features not before met with in gauging slowly-moving streams, are here presented, demanding modifications of equipment and procedure that can be learned only by the training of actual experience.

As the problem of storage reservoirs needs for its solution not only measures of the waterflow of the streams, but also the addition made to the water supply by the monthly rainfall, and the loss from evaporation, the collection of observations and statistics upon these subjects is recognized as of corresponding importance. To obtain this and other data a system of climatic observations is being organized.

To obtain rainfall observations in sufficient number to make a study of the rainfall distribution as related to and affected by local topography, the co-operation of the residents to whom the benefits of the work will accrue is solicited in making the necessary observations.

The report of Major Powell closes with an estimate of expenses for the coming year, and, in accordance therewith, an increased appropriation, viz., 250,000 dollars, has been made by Congress for continuing the work. As yet no lands have been designated as irrigable lands and reserved from settlement under the last clause of the bill providing therefor, but the rapid influx of speculators upon any land that is reported as suitable for irrigation work emphasizes the probability that this will be done at an early date.

This report, together with Senate Ex. Docs., Nos. 134 and 163, 50th Congress, 1st Session, setting forth the needs, scope, utility, and results to be attained by such an irrigation survey, and a report by Major Powell in reply to a Senate Resolution calling for information as to "the extent to which the diversion of the waters of the Platte and Arkansas rivers and their tributaries in Colorado for irrigation affects the flow of these streams in the lower valleys during the growing season, and what measure can be devised to increase the flow of water in those streams during such season," constitute the Congressional reports on this subject made during the past year, and are the augury of a continuation and extension of that policy of the general government for the development of the great west which has found expression in the passage of the homestead laws and railroad grants of the past fifty years. The limit of profitable homestead lands has now very nearly been reached, and the present laws are inadequate for the further settlement of that immense tract of arid country that can be rendered habitable only by the artificial economy of the water supply. The statement that two-fifths of the whole area of the United States is dependent on irrigation for its agricultural operations will give some idea of the magnitude of the enterprises that await to be developed, of the vast tracts, now barren of vegetation, that invite the transforming touch of the plough share and the pruning hook, and of the abundant harvests that will thereby be added to the wealth of the American people.

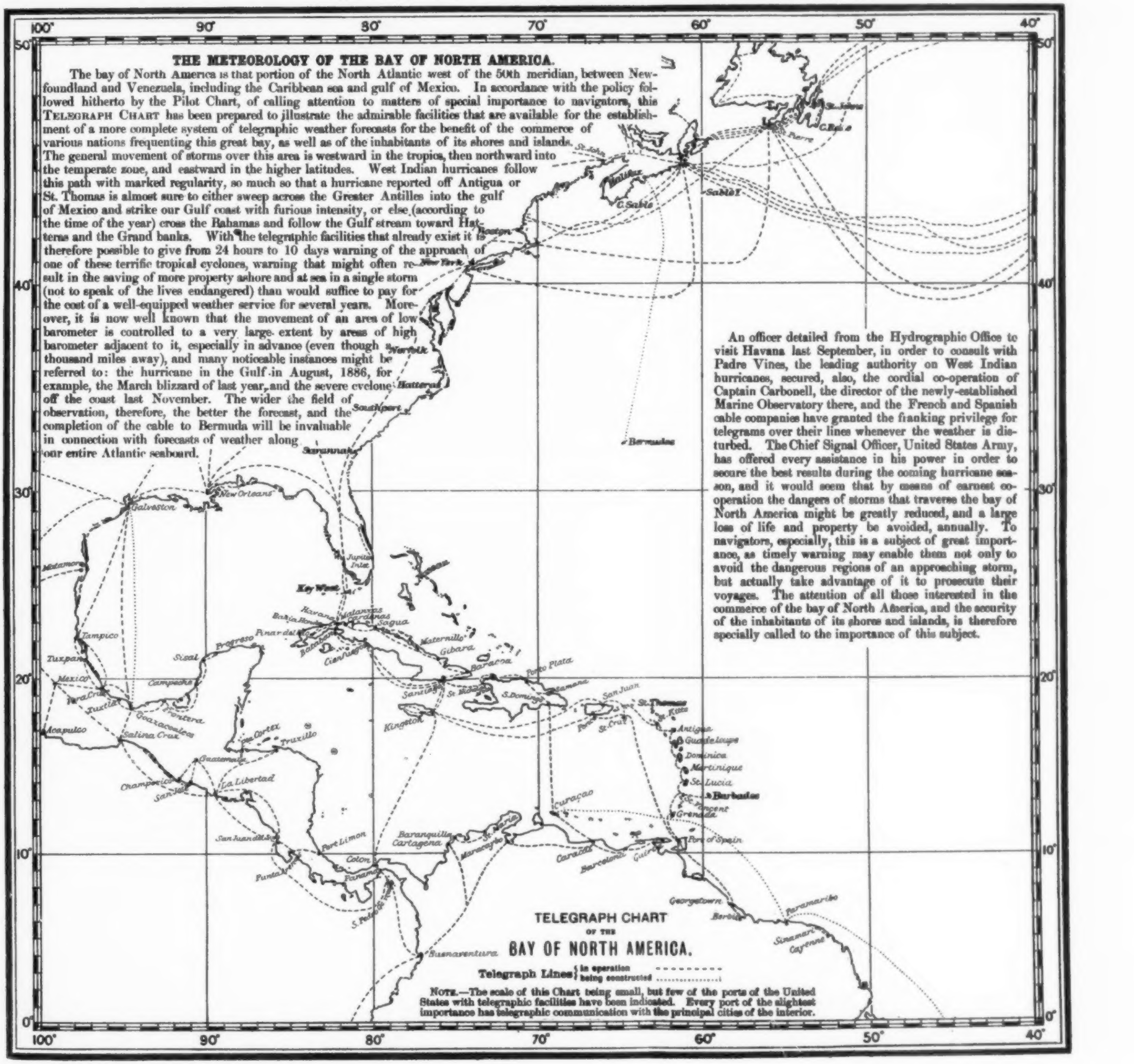
C.

SUPPLEMENT TO THE AMERICAN METEOROLOGICAL JOURNAL.— APRIL 1889

From the Pilot Chart of the North Atlantic Ocean for April, 1889.

THE METEOROLOGY OF THE BAY OF NORTH AMERICA.

The bay of North America is that portion of the North Atlantic west of the 50th meridian, between Newfoundland and Venezuela, including the Caribbean sea and gulf of Mexico. In accordance with the policy followed hitherto by the Pilot Chart, of calling attention to matters of special importance to navigators, this TELEGRAPH CHART has been prepared to illustrate the admirable facilities that are available for the establishment of a more complete system of telegraphic weather forecasts for the benefit of the commerce of various nations frequenting this great bay, as well as of the inhabitants of its shores and islands. The general movement of storms over this area is westward in the tropics, then northward into the temperate zone, and eastward in the higher latitudes. West Indian hurricanes follow this path with marked regularity, so much so that a hurricane reported off Antigua or St. Thomas is almost sure to either sweep across the Greater Antilles into the gulf of Mexico and strike our Gulf coast with furious intensity, or else (according to the time of the year) cross the Bahamas and follow the Gulf stream toward Hatteras and the Grand banks. With the telegraphic facilities that already exist it is therefore possible to give from 24 hours to 10 days warning of the approach of one of these terrific tropical cyclones, warning that might often result in the saving of more property ashore and at sea in a single storm (not to speak of the lives endangered) than would suffice to pay for the cost of a well-equipped weather service for several years. Moreover, it is now well known that the movement of an area of low barometer is controlled to a very large extent by areas of high barometer adjacent to it, especially in advance (even though a thousand miles away), and many noticeable instances might be referred to: the hurricane in the Gulf in August, 1886, for example, the March blizzard of last year, and the severe cyclone off the coast last November. The wider the field of observation, therefore, the better the forecast, and the completion of the cable to Bermuda will be invaluable in connection with forecasts of weather along our entire Atlantic seaboard.



TELEGRAPH CHART OF THE BAY OF NORTH AMERICA.

Telegraph Lines { in operation - - - - - - - - - -
being constructed . . . .

NOTE.—The scale of this Chart being small, but few of the ports of the United States with telegraphic facilities have been indicated. Every port of the slightest importance has telegraphic communication with the principal cities of the interior.

An officer detailed from the Hydrographic Office to visit Havana last September, in order to consult with Padre Vines, the leading authority on West Indian hurricanes, secured, also, the cordial co-operation of Captain Carbonell, the director of the newly-established Marine Observatory there, and the French and Spanish cable companies have granted the franking privilege for telegrams over their lines whenever the weather is disturbed. The Chief Signal Officer, United States Army, has offered every assistance in his power in order to secure the best results during the coming hurricane season, and it would seem that by means of earnest co-operation the dangers of storms that traverse the bay of North America might be greatly reduced, and a large loss of life and property be avoided, annually. To navigators, especially, this is a subject of great importance, as timely warning may enable them not only to avoid the dangerous regions of an approaching storm, but actually take advantage of it to prosecute their voyages. The attention of all those interested in the commerce of the bay of North America, and the security of the inhabitants of its shores and islands, is therefore specially called to the importance of this subject.

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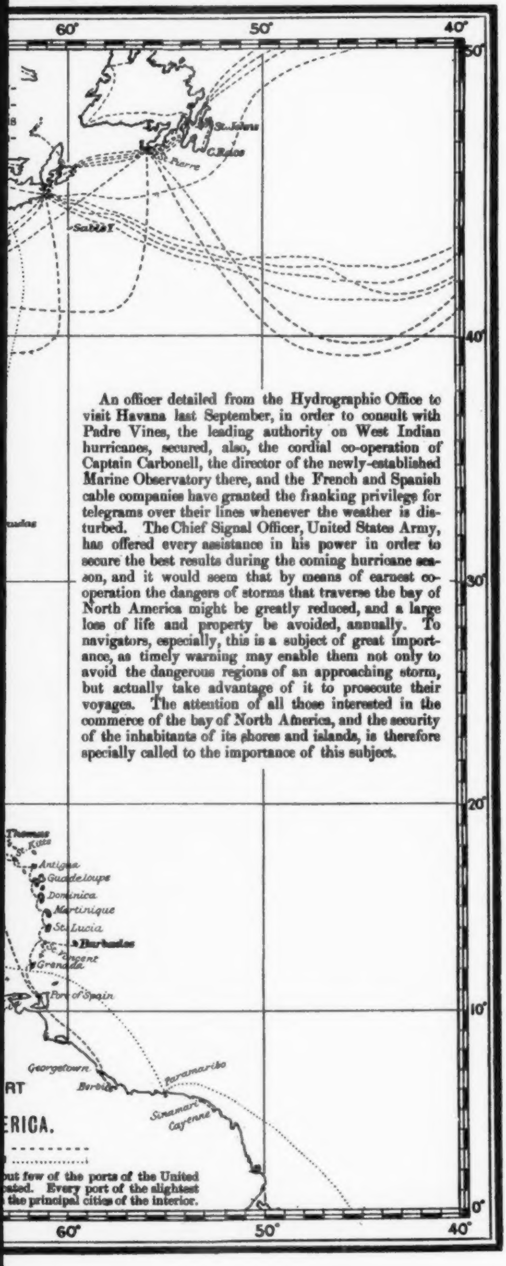

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